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LOGIC AND REASONING

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PREFACE

THIS *is not a text-book on Logic.* It pretends only to indicate a way of approaching logical studies. I have attempted to show, with the barest minimum of technical expressions, the sort of topics that find a central place in current logical discussions. Readers desiring to follow up any of these topics will, it is hoped, find the "References" appended to each chapter help them to a fuller understanding, but it will be found advisable, as a rule, to acquire a general view of the whole field before setting out to *approfondir* a "single" topic on its own account. The Bibliography at the end will suggest suitable reading for this purpose.

S. V. KEELING.

February, 1929.

Satyāmshu Mohan Mukhopadhyāya
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LOGIC AND REASONING

CHAPTER I

THE HABIT OF SOUND REASONING

"WHAT passes for knowledge in ordinary life," writes Mr. Bertrand Russell, "suffers from three defects: it is cocksure, vague, and self-contradictory." Few of us will probably deny the truth of this criticism—at least, when it is knowledge other than our own which is in question! For ourselves, our own opinions and beliefs, though we admit to an abstract liability to error, the actual commission of it is, we feel, rather an exception than the rule. And this relief we are the more ready to allow ourselves when the beliefs in question happen to be those we feel strongly about. It is hardly more difficult to be impartial about contentions that evoke no emotional warmth in us than it is to be generous with what we do not value. This, however, is only half the story. It is also true that day to day experience has at times the trick of leading us wonderingly into uneasy doubts—doubts as to whether all is well with our familiar furniture of opinions and convictions. Or, in an unclouded hour, reflection will suggest that certain views we hold are incompatible with each other. Memory, again, may revive earlier opinions which contradict our present ones on the same matter. In this way, a general query may form itself in our minds: How can we tell, in respect of any particular belief we hold, whether we are, once for all, rationally justified in holding it? And further, as moderate minds, affected with a healthy tincture of self-criticism, we may come to recognize the difficulties besetting a sincere attempt to judge soundly and reason cogently. Now, as we progress, we shall find ourselves more ready to substitute for what was cocksure, something tentative; for what was vague, something precise; and, perhaps, for what was previously pleasing or flattering, something less agreeable, if necessary. Instead of trying to bend facts parallel with our wishes and preconceptions, we shall, with less obstinacy, conform our wills to accepting the previously unsupposed if our evidence requires it. This frame of mind may involve that we may be able to "make up our mind" less frequently than we were in the habit of doing. But when we do find ourselves able to make it up, we shall have the satisfaction of a rational justification for doing so.

Now, amongst this familiar furniture of our mind —our daily beliefs and hardened convictions—some we shall recognize as being particularly due to our own efforts to "think straight," the results of our own private enterprises in reasoning. Other beliefs we shall distinguish as having been acquired by us in innumerable ways that are not due to our own efforts. And a sweet reasonableness may lead us to suppose that if the conclusions of other people are sometimes doubtful, uncertainty can scarce be uniformly absent from our own. Yet the determination to seek out and sift the evidence before passing judgment can do much to avoid the commission of error, and, hence, the risk of it. By consistent practice we may fairly hope to think more correctly. In this way, the question arises: How then can we *acquire the habit* of doing so? The disposition or willingness to do it may be an indispensable condition for its attainment, but alone it can hardly be sufficient to bring the desired result.

These two problems—firstly: *How can we tell of any belief, whether acquired by reasoning or other-*

wise, whether it is rationally justified? and secondly: *How can we learn to reason correctly?*—are obviously connected in an intimate way. The first is a problem of logic, the second is mainly one for psychology to decide. But an answer to the latter presupposes, to some extent, an answer to the former. Discovery of the means by which a habit of correct reasoning may be formed (as of any other "correct" habit) depends in part on knowing what constitutes this important feature of "correctness" in reasoning. Take the second problem first. Let us suppose for the moment that we *do* know what constitutes a correct piece of reasoning correct, and just why any piece of fallacious reasoning *is* fallacious. That is to say, suppose ourselves to know already what are the conditions which no piece of valid reasoning will fail to conform to, what are the principles that confer validity upon any conclusion we are rationally justified in entertaining. Now, these conditions, knowledge of which in itself is purely theoretical, are capable of being employed as practical rules or principles by means of which to *test* particular pieces of argument or reasoning; not that we may suppose that every argument which obeys them necessarily leads to a true conclusion, but we may be sure that no argument which fails to conform to them can possibly be sound.

It is, however, one thing to discover whether some particular conclusion to which our reasoning has brought us is a valid conclusion, and quite another thing to discover how the habit of correct reasoning may be cultivated. Logic is a science, essentially a body of *theoretical* knowledge. Its primary function is *not* to enable one to reason more correctly, but to explain the conditions of valid inference—though, of course, an understanding of these conditions may facilitate improvement in our reasoning ability. But it is a science which can be applied to particular cases of reasoning, and when so applied, we are considering its "practical" aspects. The habit of correct reasoning, then, concerns the practice of *applying* something, but what is applied remains at bottom "theoretical" or "knowledge." Consider, by way of illustration, the practical aspect of logic and the practical aspect of a language. When some grammar has been mastered, the learner begins to apply it in conversation. At the beginning he commits blunders, which can be corrected by him by reference to his "theoretical" knowledge of the grammar. With time and practice he commits fewer mistakes and attains greater confidence in his conversational powers. He feels that were the grammar of his speech called into question, he could defend it, or justify it—that is to say, he would be able to *explain* its correctness by recourse to the syntactical principles of the language. The same is the case with Logic. An intelligent mastery of its principles, an insight into the "reason why" of them, tends, when "grafted" into the habit of *applying* them to particular pieces of argument, to form a "logical mind." An expert reasoner will feel beforehand that if an inferential step in one of his arguments were questioned, he could defend its correctness by recourse to logical principles and exhibiting its conformity with them. Now, our learner's conversation may be syntactically correct, and yet open to the objection of being un-idiomatic. There is a question of using the "right" words and phrases, those proper to the genius of the language, besides the question of rightly combining them. Similarly, accuracy in the performance of inference will show that we can reason correctly, but it alone cannot secure that what we reach is knowledge. Our steps of inference may be faultless, yet the conclusion we have drawn may be false notwithstanding. From lack of fuller knowledge, knowledge more accurate or more adequate, we may have inferred our conclusion from "evidence" which is false. However impeccable our inferential process may have been, the conclusion in this case cannot be other than false.

All educative effort is in the main one of acquiring certain sorts of habit. The application of logical rules is first made consciously by the learner, much as, in linguistic study, the early stages consist in a conscious application of grammatical rules to "exercises" or examples. But with practice and time our thinking tends to become more accurate "on its own account." The rules are no longer *consciously* applied, but are felt to be "in the background" ready to be summoned up at our bidding. This, then, would seem to be one characteristic of the thinking of logically trained minds. It is *not that they consciously apply rules before drawing conclusions,* but that, their argument completed, their conclusion asserted, they immediately see (or can easily come to see) just which logical principles that argument depends upon for its validity. In other words, *criticism* of the validity of an argument consists in the explicit application of logical principles to that argument, but correct *reasoning* does not involve the explicit recognition and application of logical rules during the reasoning process itself. Even a logician, whose special business it is to analyze and formulate logical principles, does not, when thinking about other matters, explicitly call those principles to mind and consciously apply them during the course of his reasoning. He may do so, of course, after his course of reasoning is *completed* and his conclusion drawn, either to satisfy himself that he has made no slip or to defend his argument from attack. So to form the habit of correct reasoning does not mean to form the habit of holding logical principles continuously before our minds, or of consciously applying them successively throughout any actual stretch of thought-activity. Such explicit presence of rules to the mind, during the course of reasoning activity itself, would prove a real embarrassment to clear thinking rather than a means of promoting it. So one characteristic of a logically trained mind will be, not so much a recognition by that mind during its reasoning processes of a successive application of logical principles, but rather, its capacity to summon up unhesitatingly at a moment's notice, the relevant principles which are together sufficient to exhibit the truth or falsity of a given piece of discourse. And *this* application of logical rules is one that takes place *after* the actual reasoning process is finished.

What a study of logic can give us then, is, firstly, a clear understanding of the character and function of the principles on which all sound reasoning depends; secondly, an insight into the way these principles are related to one another, and, by this means, an insight into the structure of thought itself. Add to this that we give ourselves plenty of practice in the analysis of concrete arguments good and bad, and that we take the trouble to pursue this analysis in a thorough-going spirit, or what is the same thing, that we are unwilling to turn our backs on an argument until we have sucked it dry of all that is logically interesting in it—given this, "theory" and "practice," we may fairly trust to being on the highroad to acquiring a uniformly critical habit of mind and a keen eye for fallacies. And by "critical mind" is meant, not a carping or sententious manner of discussion, but a mind that is not beset with an everlasting itch to affirm where it does not know, to speculate wildly in the absence of evidence; in short, the critical mind *par excellence* is one which consistently declines to believe anything when no reason is forthcoming to show that it is true.

With such a notion of what is meant by a "logical" or "critical" mind, we are in a position to return to our original question as to how it may be formed. Some few people, it would seem, need do little or nothing to form it, for possessed natively, so as to say, of such a critical temper, they need little beyond the occasions offered by daily life to keep it "in trim." Others there seem to be who neither possess such a mind nor the capacity to attain it. Their abilities may be even outstanding in some respects, yet it seems that, to their dying day, they will never feel the "logical force" of an argument. The great majority of mankind, however, appear to come in between these extremes. Their reasons are sometimes sound and sometimes faulty, and, generally, they are quite oblivious to *why* it is the one or the other. So it turns out that they sometimes suppose themselves to have reasoned soundly when their argument is really unsound, or they come by conclusions which are true for reasons which themselves are either quite false, or else such as do not imply the conclusion they have drawn. Such muddles and confusions are not merely theoretically possible but constantly occurring in our ordinary thinking. So it is not surprising to find people usually ready to turn to some form of "authority" in the hope of setting their minds at rest, in matters where "authority" cannot by the nature of the case determine validity or truth.

So far as the *general* conditions under which the habit of correct reasoning may be formed, these will be largely those under which *any* habit may be acquired. In the last resort, the only way to acquire a habit of acting in a certain way is to try and act in that way again and again. Successful attempts tend to be specially attended to and "enforced" by feelings of self-encouragement, unsuccessful ones neglected, and in part therefore eliminated. Practice, if accompanied by a clear notion of exactly what it is we want to do, and attention to the probable causes of failure in respect of unsuccessful responses—these, enforced by our own feelings of satisfaction or discontent, will tend to secure elimination of inappropriate responses. Acquirement of skill is learning to perform accurately, attainment of accuracy is simply the elimination of inaccurate attempts, improvement is measured by the diminution of inaccurate attempts. Thus the learning of the desirable thing is simply an unlearning of the undesirable one; diminution of unsuitable responses amounts to greater frequency or constancy in performing suitable ones. Such a process of elimination, then, makes possible *fixation* of the habit. So, too, with reasoning. *Some* sort of reasoning is native to the most primitive man, but the habit of *logical* reasoning does not seem to be an inherited mode of response, but, rather, a late acquirement due to modification through learned control of our intellectual processes. Thinking we "start" with, but it is unclear, incorrect thinking. Formation of the habit of sound reasoning is largely a breaking with this inherited manner of thinking, and we break a habit by forming a counter-habit. Now, repeated exercise has a cumulative effect—the more persistently we "pull ourselves up," the more often we clearly recognize the errors inherent in particular thoughts, the less often are these types of error liable to recur. In that measure we are "learning" to think correctly.

We may also begin to discern more clearly that our thoughts are interconnected, that they have a "character" or "form" which can be separated in mind from their "matter" (or what they are "about"). Thus we come to recognize them as belonging to one or other of a few well-marked types, already distinguished, formulated, and classified by logic. We come to "spot" relations between the several thoughts or propositions as they are successively asserted in the course of our argument, and so the argument as a "developing whole" is seen to possess a sort of pattern or form. And it is a clear perception of this distinctive form or "order" in our argument which, in the end, enables us to recognize it as an instance of some general type. In this way a knowledge of logical principles is an indirect aid. It does not directly, of itself, promote or develop the *habit* of valid thinking, but it indirectly facilitates an unhesitating acceptance or rejection of any argument after it has been submitted to analysis. So we acquire a disposition to proceed in certain ways, and not in others, in the course of argument generally, no matter what be the subject-matter in question. Our minds become "predetermined" to proceed in certain ways, to reach certain sorts of results rather than others. Logical principles indirectly contributing to determine the sort of conclusions we shall reach, may then figuratively be said to "guide" or "control" our reasoning activities in some measure.

As to forming a habit of accurate reasoning, there would thus seem to be no better way of doing it than by examining critically, in the light of logical principles, particular beliefs and particular samples of reasoning, sound and unsound. Take a belief and first get quite clear as to what is maintained in it. We shall understand more fully what exactly is the import or "reach" of the belief if we inquire also what it directly *denies*. Every true belief implies the falsity of some other belief—its contradictory. Formulation of the contradictory of a proposition will often put us on the scent of a fallacy. Treat arguments in a similar way. Take a clearly stated argument and pull it apart, piece by piece. Get clear about what is being asserted in each of its constituent propositions, always distinguishing between those which function as evidential propositions and those which function as their conclusions. We should not only satisfy ourselves *that* the conclusion does really follow from what is stated in support of it, we should also satisfy ourselves *why* we think it follows. This calls for the application of the principles of logic. Then sometimes take a bad argument and treat it in a similar way. Let us satisfy ourselves not only *that* the conclusion does not follow from the evidence adduced, but *why* it does not do so. The spirit in which we might usefully begin the overhauling and reform of our reasoning is one in which we first bring ourselves to take an interest in samples of it—taken as "samples." If we can induce an interest in samples of reasoning *on their own account*, leaving in abeyance for the moment what interest we may have in the subject-matter the reasoning is about; if, as an initial and provisional measure, we can give the same disinterested attention to a faulty piece of reasoning or an irrational belief that a surgeon gives to an unusual pathological tissue; if, in a word, we can bring to bear some of that precious childish curiosity which incessantly asks the "reason why"—we may be well along the right road to our destination. When the habit has been acquired, this selective attention, born of an interest in the process and products of reasoning as such, may safely be left to look after itself. The other interest, which, before we began to take reasoning seriously, centred almost wholly in the *things* which our thought was about, may now fairly be allowed to reassert its claim to a share of our attention without risk of it usurping the due place of our newly acquired "logical" interest. For now, conscious of a liability to error through preconception or bias, and aware of the conditions that logic requires to be fulfilled, the danger of an ingrained "language-habit" conducting our "thinking" for us is considerably lessened.

To sum up: consistently sound reasoning in the vast majority of cases is an outcome of habit. Such a habit can be acquired, but like all habits, it presupposes "interest"—an interest in "correctness." Its formation depends partly on an attitude of mind which submits willingly to the constraint of facts. Such a mental attitude in turn presupposes a widening of our normal range of interest—a temporary withdrawal of it from the "things" our beliefs or reasonings bring before our minds, and a concurrent concentration of attention on the structure and steps of arguments in themselves, all directed to the dual end of discovering, firstly, what evidential presuppositions those arguments make, and, secondly, whether those presuppositions can be maintained. For a while, procedure along these lines will inevitably result in more frequent suspensions of judgment. It will not favour the collecting and amassing of convictions. And none of us would, perhaps, be any the worse for following Descartes' advice of trying to doubt experimentally once in our lifetime our commonly received beliefs so as to discover what basis they have in reason and fact.

Our procedure, then, in argument and thinking generally will be (i.) negatively, to withhold judgment where we are not clear of what precisely is being maintained, or what exactly is required to prove it; (ii.) positively, to be willing to consider arguments and beliefs on their own merits, with a view only to their rational justification or rejection; (iii.) to acquaint ourselves, by a study of logic, with (*a*) the conditions which no valid argument may contravene, and (*b*) with the methods available for procuring evidence in support of beliefs, whose truth is under examination. The significance of these suggestions, however, will become clearer if we turn now to consider two central topics around which all accurate thinking turns—namely, *Belief* and *Inference*.

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CHAPTER II

BELIEFS AND FACTS

THERE cannot be clear thought unless clear meanings attach to the terms we use. And since our purpose in this booklet is to reach some clear thoughts about clear thought, we cannot do better than inquire what meaning we attach to such words as "belief" and "inference," "fact" and "knowledge," which figure throughout these chapters. Such words are not unambiguously used in common speech. Logic has succeeded in conducting its inquiries for the most part without creating a special terminology. It has taken over as its technical terms words from common speech, but it has attempted to assign a preciser signification than is usually accorded them there. Now, when words are used in more than one sense, referring now to one thing and now to another, it is seldom that the things referred to are totally unrelated to each other. (When they are, such words, of course, form the stock-in-trade of the punster.) The variant meanings are, in fact, usually connected rather intimately, and it is just for this reason that their difference of meaning comes to be overlooked. Sometimes, of course, verbal ambiguities arise through useless innovations which spoil the vocabulary of the King's English, as, for example, when "phenomenal" is allowed to mean "exceptional" or "extraordinary," or "transpire" is used for "occur" or "happen."

But often (far more often than is usually supposed) verbal ambiguities are due to the fact that language is not so analytic as the thought which seeks expression through it. Language is primarily concerned with the business of living and was invented to facilitate it. And as the sort of thinking occurrent at the primitive level is not in principle exceedingly complicated, it is not surprising to find that our language may prove inadequate for purposes in which accuracy of statement is of primary importance. In so far as our activities in modern life have become " specialized," in proportion as they depend more and more upon the prior formations of judgments—that is, in so far as judgment has increasingly to precede action—in so much that the ordinary individual and not only the professional scholar is expected to have rationally justified views on political and economic disputes, on questions of war and peace—so far as this is a normal requirement and not an exceptional one, a great deal more reasoning of a theoretical or hypothetical character is required of everyone than in the days when Adam delved and Eve span. To some extent our thought in these more complicated issues has been aided by piecemeal invention of new words or " technical terms " on the part of those whose special business it was to study the problem, these new verbal inventions having passed in some measure into the vocabulary of daily life. But the creation of such new terms with well-defined meanings only facilitates clear thinking and the clear expression of it to a limited extent. We tend to think in significant " wholes," to come by our meanings " in the lump," so to speak, and only *afterwards* to sort it out piece by piece and express it successively by significant noises or visible marks. That is to say, we do not work up to a complex conception by successive additions of significant words, and so in the end reach, so to say, a cumulative complex meaning. Our ordinary thinking appears to proceed, much like our ordinary perception, by successive discriminations within a slowly changing and developing field of meaning. And we not only tend to think in " significant wholes," but we also express what we have thought in a similar fashion—that is, in complete sentences and not by single words, each having its fully summed-up meaning in separation from the whole sentence or descriptive phrase of which it is a component. The supposition that the terms of a proposition stand to the proposition, or that the words of a sentence stand to the sentence, as the letters of a word stand to the word, is an unduly naïve view of the structure of thought as of language. So, while recognizing that language is certainly a vehicle of thought, and that well-constructed language with a suitable vocabulary can facilitate the conduct and clarify the expression of thought, the fact also remains that language, in a sense, clogs its free and swift passage and, being at best an imperfect instrument of interpretation, creates some confusions even though it dissipates others.

Insufficient attention to the meanings of such words as "belief," "knowledge," "opinion," "certainty," "truth," "validity," "fact," "inference," and their cognates has been the occasion of blunders and confusions. For instance, we speak of believing when we do *not* claim to know, also believing *because* we know. We speak of a "considered opinion" when it is enforced by grounds, of a "mere opinion" to indicate alleged absence of them, and we contrast opinion with knowledge as implying the relation of unplausible to probable, or that of probable to certain. Again, in claiming *to know* something, we may mean "to have met an instance of it," or "to be in possession of a piece of knowledge," and here by *"knowledge"* we may mean "something which most people are agreed upon" or "something which nobody *has* so far denied," or "something which nobody possibly *can* rationally deny" or "something which experts tell us," or are supposed to tell us. "Fact," too, is a term particularly confused in ordinary usage. We are told that such and such a fact is "not true," not in the correct sense that this statement *can* bear—that is, that "true" cannot be significantly asserted of facts, any more than "amorous" can be asserted of triangles—but in the sense that there *are* true facts, but the fact in question is not one of them! Any other string of terms central in discussions on, say, political or social issues would serve equally well as these to illustrate that the use of the same term by different persons, and by the same person at different times, is unduly varied. But all these are variations which are recognized by language as being permissible and idiomatic usages. No such common employment contravenes any grammatical rule. But it is only too desperately easy for logical rules to be contravened by them, and so fallacies *in dictione* arise.

It is a golden rule to work out one clear meaning for each important and central term throughout a discussion, and to adhere to that single meaning whenever the term is used. And straightforward meanings can be found for all the terms we want to use, which will work quite conveniently in practice. In this way we can at one and the same time secure clarity in our thinking and come to see how beliefs, opinions, knowledge, truth, and validity are connected with one another. Consider what is meant by the following: a proposition, a belief, an hypothesis, a conviction, an explanation, an assumption, an assertion, a supposition, a truth. Each word denotes something different. But there is, over and above this factor of difference between each, an element that is common to each. If we start with the first idea, that of proposition, we shall find we can express conveniently and simply these factors of difference and that common element. For a proposition is simply that which is *proposed* in thought for acceptance or rejection, as being either true or false. We may define a proposition as *whatever is true or false,* adding that whatever is neither cannot be a proposition. So, "Plato was born at Athens" is a true proposition; "Milton was born before Shakespeare" is a false one. But we also said that a proposition was formulated in thought for eventual acceptance or rejection by the thinker; in a word, it is not that which is true or false alone, but also that toward which a thinker may appropriately take up various attitudes of mind. Thus we shall say a supposition is a proposition that somebody has *supposed* (to be true or false), where the relevant mental attitude is one of supposal or assumption. An assertion is a proposition which somebody has *asserted* (to be true or false) where the attitude adopted is one of assertion or affirmation. The past participle in all such cases indicates the attitude of mind which has been adopted towards the proposition. Now, one and the same proposition can be at one moment "*supposed*" as being true—for example, in order to see what consequences follow from its supposition—and later *asserted* to be true—for example, when the consequences to which its supposition led are found to be corroborated by fact. Here there is a change of mental attitude, but not of proposition. Likewise a belief is a proposition *believed*, a conviction is a proposition of whose truth one *feels convinced*, an hypothesis is a proposition that is *hypothetically considered* and whose verification is awaited. Thus there is a *dual reference* to a common element (the proposition) and a variable element ("attitude of mind") *both* contained in such single words as belief, assertion, doubt, hypothesis, etc.

"Beliefs," "assertions," and "truths" always have reference to actual or possible states of affairs within which several distinctions must be drawn. Believing, for instance, is a "state of mind" we all find easy to identify but difficult to define. Now, obviously a state of believing cannot occur unless there is someone to "have" it, nor unless there is something being believed while that state of mind persists. That is to say, the state of affairs that exists at that time requires us to take into account *a believer, a believing*, and *a belief*—that is, a person who is having a feeling of belief at the time in respect of some or other proposition.

Nor is this all. That person is believing either truly or falsely—that is, the proposition believed by him is either true or false—though, of course, he may not *know* at the time which he is doing. This brings in an additional consideration. The believed proposition is true or false. There is no need to indulge in rhetoric about "Truth" to get a plain meaning in our minds for this. We shall say that a proposition *is true* if there is a fact for it to correspond with, that it *is false* if there is no fact with which it corresponds. Now, we have seen that a fact is *not* something that can be true or false. There either exists or there does not exist a fact of the sort described in our proposition. So to say that our proposition about Plato *is true* means that there is a fact with which it corresponds—that is, the event which took place at Athens and was the birth of Plato. And since there was no event which took place at Rome and was the birth of Plato, the proposition "Plato was born in Rome" is false. This gives a clear meaning for the truth or falsity of *any* proposition, no matter what it is about. Every proposition purports to refer to some fact—that which it is said to be "about"—and for every true proposition there exists a corresponding fact.

"Propositions," then, form our central theme here. True propositions stand in two relations, one to the *person* who adopts mental attitudes towards them, another to the *facts* with which they correspond. And on every occasion that there is knowledge, such a whole complex state of affairs becomes actual. And we may then conveniently distinguish within it three "parts"—namely, a "subjective," an "objective," and a "factual" one. The minimum of requirements to compose a knowledge-situation are: (1) A person who is judging or asserting to be true (2) some proposition then before his mind, for which there exists (3) a corresponding fact. In the whole state of affairs in which (1), (2), and (3) are thus interrelated, the person, together with the states of mind he successively adopts throughout the period that he is contemplating, supposing, doubting, believing, or asserting the proposition, make up what we can conveniently call the "subjective factor." The proposition, in respect of which such states of mind are lived through or undergone by the person, may be called the "objective" factor, since it *is,* for the time being, *the object* of his mental attitudes. And the fact which corresponds with the proposition, and on account of which correspondence he is justified in asserting it, may be called the factual element. Now, this factual element is clearly independent of the mind for its existence in a way in which the proposition is not. If somebody did not believe or assert at a given time, it follows that nothing would have been believed or asserted then; that is, there would be no proposition. But it does not follow there would be no facts—though, of course, it would follow that such facts as there were would in no case correspond (nor fail to correspond) with any proposition.

It is precisely this distinction which gives meaning to the word "discovery." To discover a "new" fact presupposes the fact exists independently of our minds; that is, independently of whether we *know* the fact to exist or not. If it did not do so, we should hardly be discoverers, but rather creators of it. My mind does in a sense "create" the *proposition* "coal burns"; it does not, however, "create" but simply "discovers" the *fact* of coal being combustible. So the distinction between proposition and fact not only enables us to speak significantly of "discovery" (hence of "knowledge" and, further, of "progress in knowledge"), but it also explains what was meant in the last chapter by saying that in the search for knowledge we should not try "to bend facts to our own preconceptions and wishes," but to induce our wills "to conform" to facts. If the character and structure of the outer world made no such constraint upon our will in judging, there clearly would be no meaning in the phrase "rational justification." "True" and "false" would then be meaningless words, for it is entirely through this notion of factual constraint that they are definable. Before passing from the "independence" of facts, we may further notice that "progress" in knowledge depends upon the possibility of affirming more and more propositions, not, however, that number alone is the important point, as we shall see in Chapter VI. Now, possibility of affirming hitherto unknown or uncertain propositions depends in turn upon the discovery of more and more facts. These newly discovered facts may be such as either permit us to contradict some proposition which we had only considered as problematic, or which we had mistakenly affirmed, or it may warrant our affirming, in positive terms, a brand-new piece of knowledge. Discoveries in science are constantly made in both these ways, indirectly and directly, though more often in the former.

Before leaving propositions, there is a third characteristic of them, recognition of which may help to dispel confusion. People are heard to declare: "Nothing is certain in science: the scientific certainties of one century are contradicted in the next." An adequate discussion of where they are right and where they are wrong in this confused opinion would require a lengthy analysis, and probably a book on the character of scientific knowledge to itself. But this at least here may be said. At bottom their allegation usually amounts to holding that beliefs may be true at one time and yet become false at another. The answer to this is summed up in F. H. Bradley's classic statement: "Once true always true, once false always false! Truth is not only independent of me, but it does not depend on change and chance. No alteration in space or time, no possible difference of any event or context can make truth falsehood. If that which I say is really true, then it stands for ever . . . If any truth is stated so that a change in events will make it false, then it is not a genuine truth at all." This principle is a *conditio sine qua non* of any consistency in belief and of the possibility of any validity in inference. The supposed exceptions to it which people have in mind usually arise from ambiguities in the *verbal expression* of the truth. Some intrinsic part of the fact which the proposition is about is omitted from the statement which records it. An extreme case would be, for instance, the assertion: "It is fine this morning." This would loosely be said by many to be true on some days and false on others. But directly we substitute some unambiguous description for the variable "this morning" (for example, "the morning of May 1, 1929"), the proposition is seen to be definitely true or definitely false, and such as never can change. What such people really have in mind, that is to say, is not *one* proposition at all but a whole host of them—as many *different* propositions as there were different mornings referred to. This peculiarity of a proposition (that it cannot change in respect of either its truth value or its import) makes it possible for a person to hold one and the same proposition before his mind as the object of a variety of mental attitudes, also for any number of people to hold *one and the same proposition* before their minds. A dozen people may have each "his own opinion" on whether a Liberal Government in England would be beneficial to our trade. There is in this state of affairs a dozen people, but not a dozen propositions, *one* proposition only, which is before the dozen minds—though, again, there may be many dozen varieties of "mental attitude" successively adopted towards that proposition.

To sum up, the points we have to be clear about in connection with propositions are five; namely, a proposition is, (1), that alone of which truth or falsity can be significantly asserted; (2), that which is definitely either true or false (whether we know which it is or not); (3), that towards which different persons assume different attitudes of mind, such as doubting, denying, asserting, etc.; (4), that which, if true, corresponds with a fact, if false corresponds with no fact; and, (5), that which cannot change, in respect of either its truth value or its import, and can thus constitute one "common object" of a variety of attitudes on the part of a single mind, or be the "common object" of a diversity of mental attitudes on the part of an indefinite number of minds.

Up to the present we have considered beliefs, taken as "finished products," in connection with their being believed and with persons believing them. That is to say, for convenience of the moment we have treated beliefs together "in the lump" without noticing that they differ from each other besides in respect of truth or falsity. These further differences among beliefs may be reduced roughly to two—differences in their "subject-matter," and in their origin. The first sort is obvious: a belief about Prohibition is different from a belief about the Bank Rate. And difference of *origin* among beliefs is scarcely less obvious. Their "origins" may also be reduced to two—namely, those reached by *inference,* and those not so reached —consideration of which introduces our next topic.

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CHAPTER III

INFERENCE AND EVIDENCE

THE many beliefs which a particular individual holds derive from two sources. Some are reached by his personal efforts in reasoning, while others are taken over from some external source such as "tradition" or "authority." And those of the former class are usually few as compared with those of the second. Such a rough and ready division, however, is not the last word on their "origin." For the latter class include a vast number of beliefs and opinions which, though not reached by our own inferential performances, are none the less reached inferentially by others. In some such instances, the grounds or premises of the inference may now have been lost sight of. Buried in the dust of the past, they are only relics of historical or antiquarian interest, though charged in their own day with significance and emotional value. The conclusions then derived from those premises have lingered on, however, because of their practical utility or their human worth. The belief still lives though its grounds or reasons are forgotten. Those who now believe such beliefs do not do so, then, because they infer their truth from other propositions already held by them as "grounds." All the same they do suppose that with sufficient skill and patience those grounds could still be unearthed and would be found to be genuine supports for their traditional beliefs. Or, they suppose that *some* premises, the originals or others, could be reconstructed, given enough ingenuity, and these would serve to show the truth of these present beliefs. Such traditional beliefs, then, accepted in each age because everybody else accepts them, though not actually inferred are held to be *inferable* in so far as they are believed *justifiable*. Nor are such beliefs, so held, necessarily invalid or irrationally believed. All the same, we shall entertain these beliefs bearing no "credentials" with them with a justified suspicion. We shall not be unduly influenced by the fact that they are widely entertained, for the treasuries of Cultural Anthropology are crammed with amusing specimens of beliefs that were once generally held, but are now totally discredited for adequate reasons. And we shall reflect that possibly some of the "traditional" beliefs we still hold suffer from the shortcomings of their buried brothers, and that their proper place is along with them in the archives of the past.

There is a second class of beliefs which resemble these "traditional" ones in that they, too, are not held by the believer because he is in possession of evidence proving them, but differ from the former in being held because such evidence *does exist* and *is known* by somebody else. The "somebody else," besides being possessed of evidence, also claims to see that this evidence is sufficient to prove the belief in question, and we in turn are satisfied that his assertions on both these heads are reliable. A great mass of popular belief upon political, economic, and scientific matters is obviously of this order. Few have the leisure and specialized knowledge, even if the inclination, to follow detailed proofs of the scientist, but they feel none the less rationally justified in accepting his conclusions. And often they are rationally justified in attaching high probability to such beliefs, though they may sometimes be inclined to attach higher probability to them than the expert himself does, through overestimating the weight of the evidence or being unable to appreciate the probability attaching to "rival" alternative beliefs on the same subject. Everybody is under the same disability of having to accept "on trust" the existence and genuineness of "evidence" in respect of beliefs of this class, which are far more numerous than is usually supposed, and often of extreme importance to our lives directly or indirectly. The disability attaches all round and varies only in degrees: some people have to accept more of such "beliefs on authority," but everybody has to accept some. And our only reason for accounting such beliefs "knowledge," when held by people not themselves possessed of the evidence, is, so far as I can see, that our refusal so to account them would lead to paradoxical and unflattering results! For scarcely anybody would possess more than a negligible amount of "knowledge," and that mainly about relatively trivial matters. So, rather than restricting so rigorously the meaning of the term "knowledge," we extend it, so that the term may apply to that class of beliefs for the truth of which the individual does *not* himself possess evidence, providing he has grounds for believing that somebody else does. In respect of the term "knowledge" as in countless other cases, practice and convenience decide our preference for assigning to it one meaning rather than another which is equally clear.

Thirdly, there is a relatively small class of beliefs which are not acquired by means of anybody's inference whatever, and are felt to be true with complete certainty. Though we are constantly *employing* these propositions in ordinary life, we need not and do not consider them, at such times, separately on their own account as pieces of knowledge. Such are axioms or propositions which we know to be true, because we see that they cannot possibly be false. Among them are the basic propositions of arithmetic and algebra, of geometry and of logic itself (see p. 50). Propositions which are not axiomatic are, of course, often asserted to be so. An inferential belief whose truth is so clear to us that we do not think it necessary to adduce our grounds for it, is often erroneously called "axiomatic," and cocksure opinions in general have this virtue unjustifiably claimed for them.

Now, if we compare these three classes of belief, the last is easily seen to differ from the two former classes, precisely in being *non-derivative* or non-inferential. That is to say, we may usefully group together those of our beliefs reached by inference (whether performed by ourselves or by others) and distinguish them from those not acquired through inference at all. This brings us to the second central topic in logic. The first was the unambiguous expression of beliefs, the second is the inferential validity of them. In the last chapter a belief was explained as being a proposition believed, and we discussed the proposition, not with reference to its acquirement or derivation, but in its relation to the mind of the person believing it, on the one hand, and to the objective order of facts and things in the world, on the other. We shall now consider the proposition believed in another connection: namely, in relation to its evidence, and in relation to the reasoning process which "draws" the proposition believed from that evidence. The condition under which a given proposition is true, and that under which it is validly inferred, though connected, are distinctly different. If the "evidence" collectively *implies* the proposition in question, then the latter is said to be *inferable* from that evidence, just because the evidence does imply it. That is to say, it is always permissible to infer a proposition if it is implied by the propositions adduced as evidence. But to say that a given proposition is *inferable* from others is not the same as to say that it is *inferred* from them. It is not "inferred" from them unless someone has actually inferred it (*i.e.*, unless someone has actually "drawn" the inference). But anybody who *had* inferred that conclusion would justify having done so precisely because the proposition in question *is inferable from* the others (that is, he would appeal to the fact that they *do imply* it). Hence, we represent the relation of implication to our minds as an *objective connection* or a factual connection which "constrains" us in the process of reasoning, prohibiting us, in view of the evidence, from drawing one conclusion and permitting us to draw another.

But there is one more ambiguity we must keep in view when speaking of "evidence." There is that which really *does* constitute the evidence proving a certain conclusion (that is, those propositions, whether discovered or not, which imply it), and there is that which is adduced *as* evidence (that is, those propositions which are believed to imply it). But what is adduced *as* evidence is not always evidence. We may mistakenly suppose B to be true, because we believe both that A is true and that A implies B—where, as a matter of fact, A does not imply B, but something similar, A^1, *does.* And we are all the more likely to be mistaken in such connections when B happens to be a true proposition. It is not an uninstructive pastime to elicit from a friend what he takes to be the evidence proving some one of his beliefs, and then set about satisfying ourselves whether what has been adduced as evidence, collectively taken, does imply the belief in question—though, obviously, it is a game which calls for an exercise of tact! This game rigorously pursued will sometimes disclose that our doubts as to whether what is adduced as evidence really is evidence at all, rest on one of the following counts. We may, firstly, doubt whether the evidence (irrespective of its own truth or falsity) does imply what was supposed (that is to say, the belief inferred from it); or, secondly, deny or doubt the truth of what is adduced as evidence itself, and therefore urge its inadmissibility (even though the belief "inferred" from it be true); or, thirdly, deny both that the "evidence" adduced is true and that the belief does follow from it. That is to say, we may doubt or deny, firstly, the *validity* of the inference; secondly, the *truth* of the "evidence," or, thirdly, both.

Let us consider the first two cases separately. The first is essentially a matter for logic to settle. The first principles of logic alone constitute the relevant court of appeal in such cases, also the final one. For if anybody disputes their authority, it can be demonstrated beyond doubt that they *are* both authoritative and final by any proficient student of logic. But validity is often confused with truth. We always *want* to draw a conclusion which is both valid *and* true, but in reasoning, as in many other connections, we cannot always do what we want. We can, by recourse to logical principles, ascertain whether our conclusion has been validly inferred or not, and if the former, we can further know that it will be true also, *provided the premises are*. Whatever is implied by a true proposition is true; but some propositions are true, though the premises of the argument claiming to demonstrate them are false. For this reason we have to distinguish between "holding a true belief" and "having knowledge," for a true belief need not be a piece of knowledge. To believe a true belief on false grounds is simply to be misled into believing something true. It does not constitute knowledge, for to be justified in claiming to *know* the belief to be true involves that we also know the grounds which imply it, and, since false propositions imply only false propositions, we can scarcely claim to have "knowledge" in respect of the conclusion in such a case. For instance, if we suspect X cheated at cards, and later, on the production of evidence unknown by us at the time, he is *proved* to have done so, there is a tendency for us to declare: "I knew all the time he was cheating." But, plainly, it is not the case that we *knew* any such thing. All we may justifiably claim is that we held a true belief.

The person who collected and pieced together the evidence against X, however, presumably not only held a true belief about X, but also came to acquire *knowledge* about him. Our true belief was not knowledge because (*ex hypothesi*) it was not seen by us to be implied by other true beliefs we had about X. All pieces of knowledge, then, are true beliefs, but not all true beliefs are knowledge. That such " slips " arise is due to our predominant interest in " conclusions " and a tendency to neglect " grounds "—the everlasting urge to " get somewhere." Another witness to this primary interest in beliefs to the neglect of " evidence " is to be found in the actual *order* of our ideas throughout an argument. It is almost invariably the case that we assert our belief *first* and cast around for plausible grounds to support it *afterwards*. The psychological order of ideas in our thinking betrays the directive influence of our " interest." Nor is this objectionable —provided that we *do* look for confirmatory evidence afterwards, and provided that we stop to examine whether what we have proposed as evidence is really adequate to prove the belief. That is to say, we may, by recourse to logical principles, satisfy ourselves about the *validity* of an inferred belief, but not of its truth, though we may be assured of its truth also, provided we are already assured of the truth of the evidential premises. But, of course, it is precisely these that we often doubt, and this now brings the matter into line with the second case.

If we do not know whether our evidential premises are true, can we be rationally justified in claiming to *know* our conclusion? The answer to this depends in part upon whether our premises are taken as being certainly true or as only probably true. Our evidential premises will include at least one generalization, and whether this is a proposition we know with certainty or only with probability will depend upon its subject-matter—" what it is about." In the vast majority of cases it will not be known to be certain, but only to be probable, though the probability in respect of its truth may be so high as completely to warrant our "acting" upon it. That is to say, by far the majority of generalizations usually employed in arguments about social, legal, or scientific matters are reached inductively and are at best only probable. This does not mean that they ought to be mistrusted or considered "flimsy," but the very contrary. All depends on the degree of probability that may be rationally attributed to them. If the probability of their truth is high, then it would be distinctly *un*reasonable to refuse to accept them. Probability is not certainty, and we must be prepared to accept on principle any belief for which evidence is forthcoming to show that it is more likely to be true than false. Half a loaf is better than no bread.

Probability is the guide in practice, and it must be the guide in seeking truth also. And since a vast proportion of what ordinarily passes for certain knowledge is at best probable belief (or probable opinion), we must allow its entry into the garden of knowledge. For if we refuse we shall incur the paradox noticed on p. 30—namely, that almost everyone's "knowledge" will be curtailed to an impracticable degree. For generalizations that are entirely "uncontaminated" with probability are mostly confined to a few propositions of pure mathematics and of logic. There can be little hesitation, then, in agreeing that probability is the "rule of life"! And since it would be so undesirable to define the useful word "knowledge" in such a way as to involve that the vast majority of our experts, authorities, leaders, and dignitaries of all sorts, had little or none, we shall not restrict it so as to exclude "probable belief." Now, a careful reader may feel that this modification is permissible only at the cost of going back upon the distinction just drawn between "knowledge" and "true belief." But I think this feeling is groundless. For it is quite possible to hold a true belief without any grounds for it at all, from causes—for example, irrational prejudice, dislike, etc.—which do not constitute any evidence whatever. But it is *not* possible rationally to hold a probable opinion without grounds. *Propositions that are entertained as probably true require grounds no less than those held as being certainly true.* In the former case we do not require the "evidence" to convince beyond possible doubt as we should in the latter, but we should none the less be rightly entitled to discuss whether the evidence adduced did or did not constitute grounds for the probability of our conclusion. That is to say, probability is always relative to evidence, and this usually tends to be overlooked. We often say that something or the other is "probably true," just because we are not aware that it is certainly true. Assertion of probability, however, is not justifiably advanced on that ground alone. We are also required to exhibit the evidence which secured probability for a probable conclusion—no less than we are required to adduce evidence securing certainty for a certain one. And it is precisely the evidence that secures a positive measure of probability (determinate or indeterminate) for our probable opinion which also secures the *im*probability of the contradictory opinion, and so rationally justifies us in believing the former rather than the latter. The higher the degree of probability secured for the probable opinion by our evidence, the higher the degree of improbability attaches to its contradictory being true. So the probability or the improbability of any particular belief being true depends upon evidence. If we have literally no evidence to show that a proposition hypothetically entertained is either probably true or probably false; if available knowledge on relevant subject-matter does not allow us to accord any measure of probability whatever to our proposed opinion, so that it appears to be a *mere* opinion quite uncorroborated—then we shall properly describe it as *possibly* true, not probably so, and the opinion will be spoken of as a possible one, and not a probable one. To assert probability of the truth of a belief always entails that the asserter is possessed of evidence which rationally justifies him judging the belief more likely to be true than to be false.

If one of our evidential premises be probable, the belief affirmed on the strength of it can, at most, be only probable, and never certain. A proposition which is known to be certainly true for adequate reasons, cannot, as we saw, turn out in the end to be really false, whatever be the character or importance of any further knowledge we may acquire on relevant subject-matter. But we may believe *with rational justification* that a certain proposition is probable when in fact it is really quite *false*. And later discovery of the falsity of our belief (when further evidence has come to hand) by no means shows that we were previously *irrational* in assigning probability to it. It does not even show that we were *mistaken* in doing so, for the degrees of probability which may be rationally affirmed of one and the same proposition at different times, in view of different sets of evidence, will vary. Probability of the truth of a belief depended, we saw, on the collective character of the evidence adduced in its support. But the evidence may "change" in several ways, so it follows that the probability rationally assertible of one and the same belief will "fluctuate" with it. Or, to state the case more correctly, some of the evidence which once secured a given measure of probability for the belief may since have been proved false; or itself less probable than was supposed, or, again, "new" evidence may have been discovered—in which cases there is not strictly one set of evidence which "changes," but necessarily different sets of evidence, which will rationally justify our assigning lesser or greater degree of probability to the truth of our belief. So we can see, in the light of this "dependence upon evidence," that, *e.g.*, the phlogiston theory in chemistry may have been quite a respectable belief in its own day, having due regard to the state of relevant chemical knowledge at the time; but the same belief, in the light of different evidence available to-day, would no longer be a reasonably probable belief. So our ancestors will not appear particularly foolish to one who knows the character and quantity of the evidence with which they were confronted in their own times, but only to one who is ignorant of this, knowing only the evidence as it stands to-day. A modern scientist who knows both the evidence at present available in respect of an hypothesis and sufficient of the history of science to which it belongs, is frequently able to see the eminent "respectability" of a now-discredited hypothesis, in view of the evidence available in the day it flourished.

The "reasonableness" we spoke of at the beginning, then, is based, not on any *sentimental* opposition to dogmatism or cocksureness, but upon a *rational* insight into the essentially relative character of the mass of ordinarily accepted knowledge. Positively expressed, we see a clear sense in which to speak of the "progress" of knowledge, so we are prepared to take a second step towards answering the declaration: "Nothing is certain in science: the scientific certainties of one century are contradicted in the next." (The first was taken on p. 25.) First of all, so far as we are referring to any of the empirical sciences—that is, excluding such formal ones as pure mathematics and logic—it is untrue that their successive stages of development are marked by the acquisition of "certainties." At most, probabilities are secured for their conclusions, though, of course, these may be exceedingly strong in some instances. And since the probability secured attaches to the conclusion—not absolutely or unconditionally, but relatively to certain evidence adduced—there is no contradiction involved in a belief being *à la mode* in the science of one century and discredited in that of the following. For properly interpreted, scientific conclusions are not to be represented as absolute, unconnected propositions, but as being intimately inter-connected, through and through relative to *other* propositions, both for their significance and for their probability, so that it is only within a system of definitions and evidential data inductively reached (directly or indirectly) that the "conclusion" acquires its rational probability. Of course, what we always *want* to assert is not that "the belief B is rationally probable in relation to the collective character of the set of evidence, X, Y, Z." We *want* to assert "B is true," unconditionally and without more ado. But our naïve wishes in the matter must be brought into conformity with the conditions under which knowledge is possible of attainment. We must not demand certain knowledge where, in the nature of the case, only probable belief is to be rationally expected.

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CHAPTER IV

THE ANATOMY OF ARGUMENTS

Rapidly reviewing the ground covered in the last chapter, we may now usefully take stock of our present position. Roughly it is this. We have knowledge only when we are possessed of beliefs that are probably or certainly true along with evidence which secures that probability or certainty for them, leaving aside for the moment non-inferential knowledge. So, fundamentally, to *possess* knowledge, or to know, is to have true beliefs and adequate grounds for them—that is, grounds which either imply them or which render their truth probable. To acquire knowledge is to pass inferentially from beliefs of which we are already possessed (and which now function as evidence) to a further belief or beliefs whose probability or certainty is dependent upon theirs. So attainment of knowledge is always a passage from belief to belief, from the known to the relatively unknown. And reasoning is always a *process* performed in our minds, or a passage in thought: propositions are not processes of mind. They are whatever can play the rôle of a "starting-point" *from* which the reasoning process "begins," or of a "destination" *at* which it arrives. And one and the same proposition may function, in different arguments, as evidential premise for one reasoner and as conclusion for another, or as conclusion at one stage of an argument and as evidential premise at another for the same reasoner. That is to say, argument is "continuous" in the sense that we use *as evidence* at a later stage what we have concluded *as knowledge* at an earlier one. Further, these inferential steps will be said to have been *correctly per-*

formed, or *valid*, when they have passed from and to certain beliefs, none of which fail when taken together to satisfy logical principles. The whole business of logic, then, turns about propositions and their mutual relations, on the one hand, and about inference, on the other.

Propositions and Inference, then, are selected as the really central topics for exploration. All else we may encounter in studying logic has been introduced to facilitate, or to make possible, explanation of the conditions under which propositions are certain or probable and inference valid. In so far as we are chiefly interested in arguments taken for themselves as specimens of reasoning which lead to knowledge or error —that is, in so far as our attitude towards the argument is predominantly a " logical " one—our attention will be concentrated in the main on two matters. Ignoring for the moment the " material " import of the propositions composing an argument (except in so far as is necessary in order to understand what is really asserted in the propositions), and having regard to the formal character of the evidence, logical analysis will consist in discovering, firstly, whether one of the alternative sets of conditions, which must be satisfied if our conclusion is to be valid, *has* been satisfied in each particular instance, and, secondly, whether generalizations among the evidential premises have been well established. There is one requirement common to all the alternative sets of conditions that different types of argument must satisfy if their conclusion is to be true—namely, the requirement that at least one proposition in the evidence should be a generalization. So the exploration of the second point naturally follows as a result of having performed a thorough analysis. Consideration of the second point is usually marked off as a distinct inquiry from the first, and falls within that division of Logic called Induction or Scientific Methodology. This will be taken up later (Chapter VI.). For the moment we will restrict our attention to matters falling under the first consideration.

Any argument is made up of propositions, one of which is called its conclusion, the remainder collectively composing the evidence. (Sometimes, when argument is said "to prove" a certain conclusion, the term "argument" is understood not to include the "conclusion," and amounts to what I have here called "evidence.") Now, obviously, arguments may differ in many respects: one will be longer or shorter than another, or will be "materially" different from another in that its premises are about different terms, or one may be formally different from another. One argument, for instance, may wholly consist of affirmative premises; another, of affirmative and negative ones mixed; a third, wholly of generalizations; a fourth, of generalizations and particular propositions mixed, and so on. So, clearly, the conditions that must be satisfied for a valid conclusion from evidence of one type will be different from those to be satisfied by evidence of another type. Hence we speak of "*alternative* sets of conditions" in connection with formally different arguments, these alternative sets being each deducible from the most general, or fundamental, conditions of validity.

Now, we could, given time, interest, and skill enough, construct a logic for ourselves, providing also that we had seized with sufficient clearness exactly what was required to be done. Aristotle himself performed such an analysis and formulated a system of logic on the basis of it. This system underwent no really radical change until comparatively recent times. Two centuries ago Kant could declare that Logic had not been compelled to retrace a single step since it came complete from the hands of its founder Aristotle. However this may be, much has since been changed, and Kant's view certainly cannot be reaffirmed to-day, especially having regard to the radical innovations that have emanated from Cambridge within the last thirty years. Documents relating to the actual methods Aristotle used in constructing his logic, if ever they existed, are no longer extant, but we may well conceive one possible line of approach that could be followed in formulating generalizations expressive of the conditions that valid inference must satisfy. Clearly, we cannot arrive at valuable generalizations of any sort unless we have before us ample, well-assorted, and fairly representative specimens of the material to which the generalizations are to apply. So an analysis directed with a view to reaching an adequate and comprehensive grasp of general principles underlying correct argument will need for its material correct arguments themselves, since what we should be seeking to determine is simply what is common to all such arguments *as correct*. We may, then, imagine ourselves going to work at the beginning in some such empirical way as this. First, we should amass many specimens of correct arguments drawn from widely different sources. Those taken over from daily discussions on life and affairs would probably need restatement so as to avoid verbal ambiguity or unclearness, for unless the language expressing the thoughts is clear, we can scarce expect the thought itself to " shine through " in its formal purity. Other arguments we should take over as data—for example, particular reasonings in elementary geometry, natural science, economics, etc.—would be less likely to require revision, for they are already, for the most part, products of clear thinking. In this way we should have assembled some representative data—namely, a number of conveniently short arguments, all clearly stated, and the work of analysis could begin. The object in view would be to exhibit what we might comprehensively call the *anatomy* of these arguments, neglecting so far as possible their " flesh " or subject-matter, and concentrating attention upon their "skeletons" or form. Now, since arguments are composed of propositions, the *form* of our propositions will determine that of the argument into which they enter. (We may define more precisely what we mean by the "form" of a proposition as that which remains unchanged in it when every constituent term of it is replaced by another.) Having divided our arguments, one by one, into consecutive propositions and separated those collectively constituting "evidence" from their conclusions, we shall proceed to notice (i.) the relation in which the evidential propositions stand to their conclusions, and (ii.) the different forms of the several conclusions themselves, each considered on its own account. Now, these conclusions, taken separately, will be seen, on inspection, some to resemble others, and some to differ from those others. Within the groups which differ we shall find further types of resemblance, and so, continuing this process of noting identity where identity is discoverable, we shall be led in the end to reduce *all* our conclusions to quite a few irreducible types. And there is little doubt that these types will be such as traditional logic symbolizes by the formulæ—(1) SaP, (2) SeP, (3) SiP, (4) SoP, or to types corresponding to them. This amounts to saying that our conclusions will be either *general* beliefs to the effect that (1) everything that is *s* also is *p*, (2) everything that is *s* is also *not p*; or they will be *particular* beliefs to the effect that (3) some things which are *s* are also *p*, (4) some things which are *s* are also *not p* (where the letters *s*, *p*, in each case stand for attributes). Or, concretely they will be beliefs of the same type, as, for example, (1) "Whatever checks exportation encourages importation," or (2) "No hydrochloric acid contains oxygen," or (3) "Some fungi are poisonous," or (4) "Some articles do not rise in price when taxed."

Now, if we arrange our arguments into four groups, according as their conclusions are of one or another of the types just enumerated, and consider each group in turn, we shall see, by inspection and comparison, in what respects any one argument resembles each other in its own group, and in what respects it differs from a representative argument in each of the other groups. And by proceeding similarly, we shall come to see in what respect any one group differs from each of the other groups. And these general resemblances and differences that we distinguish, once formulated, can be used as the principles or conditions which each class of argument satisfies. For our procedure will have yielded us not *inductive* generalizations about each class of argument, but intuitive or certain insight into the necessary and constitutive conditions of *all other* arguments we might meet with in the future, provided they conform to one or other of our four main groups. The performance of this intuitive act by means of which we are enabled to pick out those common and constitutive conditions, may be further facilitated by comparing any argument of one of our valid classes, with an *in*valid argument having a conclusion of the same type as the valid one. The conditions formulated would be found to be either precisely those which Aristotle himself formulated (known as the principles of syllogistic reasoning), or, if apparently different, such as could be reduced to them.

The reader will find, however, that a readier way of coming by this information is to consult a textbook on the Conditions of Mediate Reasoning. Nor do I suggest that the procedure outlined would be the best way of setting about the discovery and formulation of logical principles, but it would be one possible way, and it serves to illustrate what is meant by saying, firstly, that there are such fundamental principles "inherent" or "implicit" in our reasoning, and, secondly, that these are disclosed to discriminative inspection as intuitively certified, and not inductively so, and therefore as being, thirdly, certain and not merely probable in respect of their truth.

Or, again, we might proceed in an inverse way from this. Instead of fully fledged arguments, we might start our inquiry with "bare conclusions," true or false, and then cast around for evidential premises that prove them. If we took a formally different type of conclusion in turn, and adduced suitable evidence for each, we could then proceed to analyse the completed argument, as before, noting exactly where conclusion depended upon premise, until, by an exhaustive exploration, we had fully accounted for the formal character of our conclusion by that evidence alone.

Now, there are two such conditions of a quite general nature that merit our notice. General beliefs (of the form SaP, SeP), we should find, require their evidential premises to be general also; while particular beliefs (like SiP, SoP) would require their evidence to contain at least one generalization. Evidence consisting of two particular propositions can prove nothing, nor is a conclusion inferable from two negative propositions, whether general or particular. Turning to our evidential propositions, we shall find that if any pair of them are such that they have no term in common, nothing is inferable from them conjointly which is not inferable from each separately. But if there is one term common to them both, then something *may be* inferable from them conjointly over and above what is inferable from each separately. This common term relates the terms that are not common to both propositions, hence the connection between the diverse terms so established when explicitly stated is simply the conclusion implied by the propositions. The conclusion is inferable in certain cases because the pair of propositions are inter-related (through the mediation of the common term) in precisely the way the conclusion states. If the diverse terms are, as a matter of fact, related to each other in a definite way, then it is permissible *to state* that they are so related—and this is just what the conclusion *does*. And this simply amounts to saying that it is permissible to infer what is inferable. Presumably no one would contest this, any more than one would contest that it is permissible to desire what is desirable!

Now, it is one of the characteristics of a science that it attempts to explain its data from the fewest possible principles. Some of the clearest examples of this are found in what mathematicians call an "elegant" proof. A checkmate at chess, for instance, which is effected in the fewest possible moves and with the fewest possible pieces is preferable to a "mate" secured by advancing the whole of one's "heavy artillery" and by more numerous moves. This sense of "elegance" in proof and explanation is natural to a mind that habitually works with material that can be exactly defined or precisely measured. It is no piece of pedantry, but symptomatic of an individual's sensitiveness to precision and his recognition of the finality of fact. He accepts the facts as "final" and "binding" because they, and they alone, are the very things that are fitted for acceptance. Now, this device of explaining as much as possible by the least that can possibly do it may be applied to the body of logical principles themselves. Then we shall find that the several alternative sets of conditions which secure the validity of inference (p. 42) may all be derived from one more general and very fundamental principle which is traditionally described as the *dictum de omni et nullo*. It may be expressed thus: Whatever is distributively predicated (whether affirmatively or negatively) of any class or collection, may be predicated (affirmatively or negatively) of whatever is truly asserted to belong to that class or collection. This is an axiom, a proposition whose truth is self-evident and whose denial is self-contradictory. It does not furnish us with any startling information; it is, if you will, only a truism, but that is just its merit, for being such, its truth is self-exposed. The demand to furnish reasons for reasons must stop at some point, or else our regress of evidence for evidence would be endless. And where could the demand be more reasonably required to stop than at a ground which is directly seen by everybody not to require, nor to be susceptible of, proof, since its truth is evident immediately we contemplate it? Such a truth *is* a truism and is exactly the kind of truth required to meet the situation.

All the principles of deductive reasoning, then, derive from this fundamental axiom and three others called "the laws of thought." Their interest similarly does not arise from any novel information they convey to us. They, too, are truisms, but they are not trivial, for they form the certain basis of so much else that is important. The laws of thought are fundamental, necessary, formal, *à priori* principles: "fundamental," in the sense that they are derivable from nothing which implies them; "necessary," for their contradictories are impossible; "formal," in that every true belief, irrespective of what it is about, conforms to them; and *"à priori,"* since every step of valid inference presupposes their truth, while their truth itself is directly disclosed upon inspection of any fact whatever, and is not known inductively.

The description of these axioms as "laws of thought" has not unnaturally led people into supposing that they are "subjective." "Subjective," however, is a slippery word. To understand the dual rôle these axioms play, we shall require to throw our minds back to the distinction developed in Chapter II. between proposition and fact. Let us start with what is obvious: The laws of thought are true propositions. Now, we may distinguish among true propositions those which derive their importance from the actual knowledge they convey to us from those whose main value lies, not in the information they impart, but in what they enable us to "do." These latter are normative principles, which we do not merely "know," but which primarily we are said to "use" or "employ." Their function is that of enabling us to infer conclusions that *do* impart information. The description of such primitive principles as "laws," then, points to their character of being conditions of the possibility of valid reasoning. They are "laws" in the sense that consecutive thought and coherent argument presuppose their truth and are impossible if their truth be denied. In the relation to the performance of inference they are negative rather than positive in character. Without presupposing them we cannot advance by a single step of inference, yet in themselves they inform us of nothing novel nor interesting. By this it is not meant, once again, that during the course of any inferential process, we hold such laws before our minds or consciously recall and apply them before passing on to draw conclusion from premises. Retrospection of a just-past state of mind in which inferential activity was present will not necessarily disclose the presence to mind of such laws at all. They may be, though need not be, and usually are not, employed explicitly and purposively. But if a person refused to admit the validity of a certain valid argument, we should attempt to demonstrate its validity to him by explicitly appealing to relevant logical principles. If he, in turn, contested the validity of these, we should show that they were derived from the laws of thought, and these, in turn, could only be contradicted by him on pain of *self*-contradiction. These "laws of thought," then, are not subjective in the sense in which the laws of association are; they are not descriptive of the ways in which thought actually proceeds, for in invalid inference its procedure contravenes them. It is rather a statement of how our thought *should* proceed if our conclusion is to be *validly* inferred, or of how our thought has proceeded where our conclusion was validly inferred.

These laws are called those of (1) Identity, (2) Non-contradiction, and (3) Excluded Middle. (1) Identity (" A is A," " Everything is what it is "), applied to a proposition, amounts to insisting upon its *singleness of meaning* (irrespective of innumerable reassertions of it in different contexts, and irrespective of the number of assertors), and upon the *unchangeableness of its truth or falsity* (irrespective of different contexts, times, or places in which it is reasserted). Only when propositions are thus regarded as fixed and invariable can our reasoning about them be valid. During one and the same argument, each *term,* as well as each proposition, must be used with a single unvaried meaning. This singleness of meaning amid difference of context and reassertion is not, however, imposed by our minds, but is founded in " things." It is not that the mind has a peculiar tendency or bias of its own to proceed in this way—a bias which is not " corroborated " by the structure of reality or the character of fact. Just because " everything is what it is and not another thing," just because things remain identical or " the same " despite their changes, is it possible for us to assert them, at different times, to be the *same* things *differently* characterized.

(2) The principle of Non-contradiction (" A cannot both be B and not be B," " A thing cannot at the same time both possess a certain attribute and not possess it ") likewise expresses a fact about things—one way in which things are not characterized. Therefore it has an application to thinking, for it entails, firstly, that we cannot, except on pain of self-contradiction, conceive anything both as possessing and as not possessing a certain attribute at the same time; secondly, that of two contradictory propositions, both cannot be true; and, thirdly, that in asserting any proposition to be true, we are thereby committed to asserting the

falsity of some other one. Thus, every affirmation implies a denial. And this last point is not unimportant, for we cannot be said to have fully grasped the import of any proposition to which we have assented unless we have realized precisely *what it denies*.

(3) The principle of Excluded Middle supplements that of Non-contradiction. The latter declares that of any two contradictory attributes no thing can have both at once; the former declares that one of them must be truly assertible of everything whatever ("A is either B or is not B"). Applied to thought, the principle of Non-contradiction affirms that of any pair of contradictory propositions *one must be false*, the truth of the one implying the falsity of the other, while "Excluded Middle" affirms that *one must be true*, the falsity of the one implying the truth of the other. Hence, the possibility of any "middle," or third, alternative between A being B and A not being B is necessarily excluded. The possibility of affirming something to be true by denying something else to be so rests precisely upon these two principles being conjunctively employed or presupposed. And all three principles, "Identity," "Non-contradiction," and "Excluded Middle," along with the *dictum de omni et nullo*, collectively constitute the authority and basis of every type of deductive inference we may validly perform. In this way we see that the special conditions relevant to specifically different types of deductive inference are all derivable from a few primary, more pervasive, underivable principles. Hence, so far as securing validity pure and simple for our conclusions is concerned, there are no insurmountable difficulties confronting us once we have acquired possession in practice of these principles.

But to repeat, we usually want to know more than this. We are not completely satisfied in discovering that our conclusion is true provided our premises are. We also want to know whether our premises are true, for only then can we infer that our conclusion is true with probability or certainty. Now, we saw (p. 46) that *every* valid argument must contain at least one generalization among its premises, and that those arguments in which a general conclusion is drawn must contain no premise which is not a generalization. But it is not only in the performance of inferential steps that we run risk of error; we are equally liable to it through asserting generalizations to be true when, in fact, they are not. Hence, a separate question emerges here for investigation: What are the conditions under which true generalizations, fitted to serve as premises in arguments, may be obtained?

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CHAPTER V

GENERALIZATION AND EXPLANATION

Broadly speaking, generalizations fulfil either or both these offices: they constitute "termini" of our thinking, or "media" through the instrumentality of which further knowledge may be reached, and without which it could not. A generalization may be an interesting and enlightening piece of knowledge that is worth knowing on its own account, for what it tells us about ourselves and the world and simply that alone (that is, a "terminus"); or, whether interesting or uninteresting in itself, its chief importance may be a "strategic" one in that it permits us to reach further conclusions that are interesting and important. Thus one and the same piece of knowledge may have both instrumental and intrinsic value.

So far we have restricted ourselves to considering the conditions which correct reasoning must satisfy. Now, such correct reasoning must sometimes be directed towards reaching conclusions which are not simply statements about this or that particular object, but about all objects of a certain specified sort. The world is such a vast place that, although it is easy to assert propositions about every object of a certain sort in it, it is anything but easy to know whether those assertions are even probably true. The bias that the untrained mind betrays towards free and easy generalization, and its indifference to the need of evidence, shows only too desperately the risk of error hidden here. Probably no maxim of common logic is more widespread than the warning not to generalize further than the evidence warrants—not to assert something to be true of all individuals of a certain sort when experience has only shown it to be true of some of them. But the rule, theoretically recognized, is too often ignored in practice. How many popular opinions about foreign peoples, for instance (especially when uncomplimentary), are generalizations based on adequate and trustworthy evidence? Daily experience sufficiently brings before our notice the risk we run in generalization of illicitly passing beyond what the evidence justifies. Yet it is a risk we must run by the nature of the case, for knowledge vitally depends on the possibility of reaching true generalizations. Evidently our only measure to minimize the risk is to gain some insight into the character of sound generalizations and discover how they were obtained. Our most representative examples are unquestionably those found within the various empirical sciences. The results of physics, chemistry, biology, economics, and the rest, are themselves generalizations. So if we discover the common character of such general results by examining the best attested samples of them, and if we consider the methods adopted to secure them, we shall probably acquire some regulative notions to guide us in making safe generalizations for ourselves, and to help us in criticizing usefully particular generalizations we encounter from time to time.

We commonly assume, according to purpose, an attitude of inquiry to two quite different things—namely, in the first instance, to observed *facts* of some sort (as things *to be* explained), and, in the second, to *generalizations* (as purporting to be explanatory of facts). We may be confronted with a fact that has caught our interest and for which we seek an explanation. Or we may have become acquainted with what claims to be an explanation of certain facts, and seek to discover for ourselves just how it *does* explain them. That is to say, our point of departure may be either from "facts" or from "explanations," and our dominant attitude of mind will be one of *discovery* or one of *understanding*, respectively. Much as we had to distinguish the question of how habits of correct reasoning are formed from the question of what constitutes the validity of valid reasoning, so here we shall have to observe that it is one question to ask what in general is the procedure of those people whose particular business it is to reach sound generalizations, and quite another to discover what are the principles underlying their successful generalizing. So I propose three further topics, the attempt to answer which will occupy us throughout the remainder of this booklet. They are: (1) What different sorts of generalization are there? (2) How are they obtained? (3) What presuppositions do they make?

Let us consider the first. Organized knowledge in the form called "a science" is from beginning to end bent on explanation. Though scientific interest may be awakened by observation of an unusual character or some irregular way of behaviour of a *particular* natural object, the explanatory effort is always directed towards reaching an explanation that is *general*, one that applies not simply to the particular case observed, but to all other cases like it, irrespective of place and time. Though psychologically the explanatory interest may have been evoked by a particular object its theoretical reference is not confined to that object but extends to the type or class of natural objects of which that particular individual is regarded as merely a sample or exemplification. Theoretical knowledge, as distinct from practical knowledge, consists in showing how the matter to be explained follows from or is connected with other items that are already more or less fully understood. So explanation, in the wide sense of the word, is an affair of degrees, for the character of a thing or occurrence may be totally unexplained, or partly accounted for, or possibly wholly explained. This wide or popular sense of the term, however, is not always a useful one.

Just because of its "width" those who employ it fail to realize that the solving of a problem is usually a progressive business. It frequently resolves itself into a series of attempts at explaining a number of subsidiary problems—that is to say, the solution of the initial problem logically depends upon solutions to the subordinate ones being reached. It is in this connection that a defect of much "popular" interest in theoretical knowledge becomes apparent. There is a certain *naïveté* in that popular interest which ever seizes upon "big problems," which inevitably depend for their solution on explanations to subordinate problems first having been attained. Not only is this true of more speculative questions such as: "Is there a purpose in life?" "Does the mind survive the death of the body?" "Are our wills free or determined?" but it applies equally to more "mundane" inquiries, such as: "Is Prohibition desirable?" or "Can mental capacities be inherited?" Ordinarily a supposedly plain question is asked and a plain "Yes" or "No" is looked for. A problem is raised in a single question and it is tacitly assumed that its answer can be returned as briefly. These are simply indications that what is uppermost is an interest in the "subject-matter" of the question, and that this interest is uncontrolled and "unsobered" by a scientific or logical training which inculcates the practice of piecemeal investigation and attempts to approximate towards a full answer by means of successive partial explanations, rather than returning one whole, complete explanation at a blow. Nearly all popular interest centres in what are really the most difficult problems of science and philosophy, those which a reputable thinker would rather leave to the last for solution. Those near at hand, which could be more profitably discussed, remain unknown to the general reader or, if known, interest him little. The daily press conducts its controversies on a variety of economic, social, or scientific problems, which are usually formulated in an attractive though inadequate way. Contributions to these symposia are welcomed from celebrities whose opinions may be interesting, but can scarcely be considered "expert evidence" on the matter in dispute. The periodical press will solicit a boxer's opinion on "life after death" or a cinema star's views on the value of religion. Personal curiosity may legitimately be slaked by these contributions, but the methods by which the inquiries are conducted and the character of the "evidence" adduced flavour too much of the contributor's "personal reactions" to be considered serious steps towards a solution. The gathering of such "personal views" on big problems and the tendency to count heads for a verdict presuppose that explanation is a much simpler thing than it really is. Apart from supposing that personal opinions of celebrities, however celebrated, constitute real evidence at all (unless, of course, they are specialists on the matter at issue), there remains the more important objection that explanations, even when theoretically forthcoming, cannot usually be given in the terms in which the question was "set," just because it is usually "set" in too simple-minded a fashion, without due appreciation either of the character of its relevant subject-matter or of the methods which would most likely prove successful in grappling with the problem. So a more intelligent way of following up a question that interests us will be, having first made sure we understand exactly what *is* at issue, to divide it into as many departmental problems as we can discern within it. The possibility of answering these will depend upon our taking over certain knowledge "on authority" from those who have limited their attention to the sciences relevant. In the light of this clear insight we shall probably be able to reformulate our initial question more advantageously, remembering that to be able to formulate a question precisely and adequately is often to be halfway towards its solution. At this stage we shall ponder our problem with a view to discovering where are the strategic points within it, and to grasping the sort of evidence that would be adequate to answer it without, of course, assuming that evidence of the sort is necessarily forthcoming. Obviously, no rules can be given for the successful performance of these tasks: it depends too intimately upon the knowledge and acumen of the thinker. Confronted with wide or general problems, the maxim is to divide wherever possible and conquer piece by piece. In so far as explanations of subordinate problems are reached, so far is it possible to make useful judgments of probability about the wider issue, and so far are we reaching a partial explanation of it.

The explanations which form the body of knowledge or results in the natural and social sciences are the outcome of just such piecemeal attempts. The ultimate aim of every science is to reach a complete knowledge of the essential conditions that determine the character of the data proper to its field. But so complex and subtly connected are the items of this data that this ultimate aim can be accomplished, if at all, only by approximations. The general tenor of scientific explanation is to disclose increasingly more determinate and precise knowledge of the general conditions or "laws" which account for the structure and behaviour of natural objects. And the more diversely these objects are characterized, and the more complicated the relationships into which they enter, the greater is the measure of analysis and description necessary before their laws can be traced. Explanation of assigned data, then, consists in discovering which are the fewest laws adequate to explain them, and in demonstrating with respect to particular classes of objects (contained among those data) that their structure and behaviour *do* conform to, or *are* correctly described by, those laws. Stages of progressive explanation are more or less clearly marked out by the attainment of two sorts of generalization, usually referred to as (1) empirical generalizations, and, (2), explanatory generalizations or "laws" or "established truths"—not that the latter are "established" with certainty, though their probability will usually be much higher than that of an empirical generalization.

The function of an empirical generalization is not to explain but to describe. It is, however, a description which facilitates explanation, for it is descriptive of something needing explanation. And we cannot explain what we have not summarily described and formulated. That the cost of living tends to rise during and after a war is an empirical generalization based upon the family budgets of countless people in many countries at different dates. Such a generalization explains nothing, but itself needs explanation, for it stands unconnected with any body of knowledge already acquired. For the economist this generalization constitutes a problem which can be explained by connecting it with other data concerning conditions of trade among countries at war and with certain economic principles in such a way as to show deductively how such a change in living expenses should be necessitated. Empirical generalizations are the results of observations and experiments, and usually arise at an early stage in the development of a science. When they are seen to be derivative from principles of wider generality which are already known, and which already possess independently of them an appreciable probability, the problems to which those generalizations gave rise are said to have been "explained." Empirical generalizations as statements of observed uniformities or regularities of occurrence (of the form, "All observed S's have been P" or "A certain proportion of the observed S's have been P") are presumptive evidence that there is some constant condition determining those occurrences that has so far been unobserved, or which, though known to exist and determine certain other occurrences, has not yet been perceived to determine these also. Enumerative generalizations that are only based on a few random observations (*cf.* superstitious beliefs) will clearly possess a negligible measure of probability. The probability is only enhanced when careful exploration has failed to disclose any exceptions, and when the instances composing the data of the enumeration have been selected from environments, and under circumstances, as widely different as possible. Where the data of the problem under investigation is particularly complex, and where the characteristics of that data cannot be very exhaustively defined (for example, in problems of crime, population, trade, etc.), a preliminary measure usually adopted is to collect and classify statistical findings which have been purposively elicited. Graphical and other representations of statistical summaries often reveal uniformities or variations that are felt to be significant as expressions of some underlying set of conditions that are conjointly responsible for the state of affairs that the statistics represent. For instance, the average number of suicides among a certain population may be found to vary with queer regularity according to the seasons. Statistics collected over a number of years may reveal that there has been a constant tendency for the greatest number of suicides to occur at midsummer and the lowest number to occur at mid-winter, so that a graphical representation of these statistics forms an almost symmetrical wave-like curve. Here, then, is an empirical generalization which may suggest a useful research for "laws" or determining conditions, but the generalization in itself obviously explains nothing, being merely descriptive of a vast array of facts which, taken in bulk and "interrogated," are more or less "eloquent" of some or other determining conditions underlying them. So from a mere enumeration of observed particulars summarized in an empirical generalization, an hypothesis which *prima facie* might be explanatory is formulated and the researcher pushes ahead to discover facts or to make deductions which may eventually lead to their confirmation or rejection, and thus to the explanation of the initial problem. So an empirical generalization, whether it states a *quantitative* connection (for example, "statistical uniformities") or a *qualitative* one (for example, the observed, unvariable co-presence or sequence of characteristics or events), is a summary proposition. But it is not only an economical substitute for a number of singular propositions (each recording a single instance of variable or unvariable, co-presence or sequence of characteristics); it is besides this a ground from which we may pass eventually to asserting, with greater or lesser probability, the truth of a genuine explanation.

The second class of generalizations which we called "explanatory generalizations" or "laws" or "relatively established truths" are precisely statements of the conditions that determine the character of the particulars enumerated in our empirical generalization. Explanatory generalizations, then, are "termini" of successive analyses of facts that have enabled us to discover their determining conditions. To *understand* an explanation is simply to see that the determining conditions formulated in it *do* account for the character of those observed facts whose explanation was sought. This, the narrower sense of the term explanation, is that in which the sciences are themselves "bodies of explanations" so to say.

But there is a connection between the narrower and wider senses of the term that merits attention. There are several types of explanation which may be arranged in an imaginary scale according to the degree of intellectual satisfaction provided by them. Firstly, there is the sort of explanation met with in the purely formal sciences. These are demonstratively certain deductions of propositions asserting implications—that is, so-called "necessary" or "hypothetical propositions"—such as compose the body of results in pure mathematics. Explanations of this type are not results of generalizations from experiences of particular things, but are statements of relationships between properties of fundamental concepts of a certain kind. Such statements are of the form: "If certain of these concepts are related in given ways, then they will also be related in other ways." They are deductions from definitions and axioms which may or may not correspond with facts about the natural characteristics of *existent* objects, their correspondence or non-correspondence in no way affecting the validity and certainty of mathematical results themselves. Secondly, there are those scientific explanations we call "natural laws," which *do* claim to apply to existent objects and their characteristics or to natural events. They usually possess high probability, but never the certainty of the former type of explanation. Such are the "established results" of applied mathematics, physics, and, in fact, all the empirical sciences. Thirdly, there are the teleological explanations furnished by biology and the sciences of human behaviour, but absent from such natural sciences as physics and chemistry. An essential factor in such explanations is reference to a purpose or end for which, or to which, the character of the objects to be explained is adapted, and without reference to which their distinctive character must remain unexplained. Fourthly, there is explanation by reference to membership of a class or to occupancy of a determinate position in a series. An example of the former is the identification of a certain fungus by the use of a key or classification of fungi. The ordinary procedure of classifying, referring species to its genus, is equally a genuine form of explanation, though perhaps a less "satisfying" type than the former ones.

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CHAPTER VI

INDUCTIVE PROCEDURE

We shall consider in this chapter the means that have been successively employed in reaching explanatory generalizations or laws. The procedure, commonly referred to as inductive, aims at discovering and formulating the causal or determining conditions of which existent objects and their observable characteristics are the resultants. Our survey will now turn upon the question: How is the analysis of facts, observed and observable, to be conducted so as to disclose the causal factors and conditions from which they result? Natural laws in their universality and purity are not revealed directly to inspection—we may observe a stone fall, but we cannot observe the principle of gravitation. How, then, do we pass from the observed to the unobservable? What is the general character of the transition from explicandum to explication?

In its simplest form, as exemplified by sciences at a relatively elementary stage of development, inductive procedure consists of four fairly well-marked stages—namely, (i.) observation of a number of similar facts that are summed up in an empirical generalization; (ii.) formation of an hypothesis, suggested by the empirical generalization when considered in relation to knowledge already attained which bears on these or connected facts; (iii.) a deductive step in which the investigator infers the consequences that follow from the hypothesis when entertained as true; and (iv.) return to the order of observable facts and an analysis of those relevant to the deduced consequences with a view to establishing the truth or falsity of the deductions, and hence of the hypothesis itself. When an hypothesis is confirmed by phenomena that are amenable to quantitative measurement it is usually possible to reformulate its original statement more exactly. We shall, for instance, be able to substitute for a proposition asserting mere indeterminate dependence of one state upon another state, a more determinate assertion of the quantitative variation of the one in relation to the quantitative variation of the other.

Inductive procedure in research at a "higher" or more developed level of a science is essentially similar to that at its lower level, though the points of departure in the two cases may differ. For example, instead of starting, as in (i.), from an empirical generalization summing up *particular observations*, the researcher may begin with a number of *explanatory generalizations* with a view to summarizing them in turn under a wider law. Where the explanatory generalizations are themselves explained by the more comprehensive generalization under which they are brought, this latter is called a theory. But in some cases the wider law does not explain, but merely summarizes the several explanatory generalizations (in the way that an empirical generalization summarizes a number of single observations), in which cases it is referred to, not as a theory, but as a summary law. The subordinate explanatory generalizations are at only one remove from the order of observable facts, while summary laws and theories are at two removes from it. Further, the truth of theories or summary laws implies that of the subordinate generalizations, but the truth of the latter does not imply that of the former. And clearly a lower degree of probability will attach to theories or summary laws than belongs to any one of the explanatory generalizations subordinate to them.

A general risk we run at the first stage of inductive inquiry is that of "finding" what we expect to find or what would be useful in supporting a preconception we should like to see confirmed; not that we willingly deceive ourselves "with our eyes open," but for the simple reason that accurate observation is a difficult matter, something quite other than a bovine stare. Fruitful observation must be selective; there must be a ready "give and take" between eye and mind, so that we are not side-tracked by the irrelevant, confused by mere profusion, but are intelligently selective in what we single out for careful discrimination. This risk of "finding" what one expects to find, the bias of confronting observable data with preconceptions about its character, is one we are so exceedingly liable to run in the inspection of mental phenomena, for instance, that many investigators make it the ground of a standing objection to the practice of introspection as a psychological method. If anybody doubts the genuineness of this risk let him study the psychology of attention and consider the misjudgments due to "expectation" and to volitional changes in the direction of attention. We all know from common experience how recognition of an object is facilitated if we are expecting to perceive it. And we are as well aware of the corresponding demerit of expectation which induces us to "recognize" something which is not really what we suppose at all! So ingrained are these unconscious predispositions to mal-observation and to misobservation that the psychology of bias due to expectancy confirms Jevons in declaring "it is difficult to find persons who can with perfect fairness register facts for and against their own peculiar views." We tend specially to notice instances which confirm our hypothesis and unwittingly neglect to notice those that run counter to it; much less do we set ourselves deliberately to seek out such contradictory evidence.

But selective observation alone cannot discover for us the essential or determining conditions of the facts summarized in our empirical generalization. They can be discovered only after other conditions, that are jointly operative but not determinative of the particular characteristics to be explained, have been eliminated. This elimination may be one of physical separation or withdrawal, or else a " mental subtraction," as when we know the effects of the " eliminated " condition already and can, therefore, make allowance for them by regarding them as irrelevant features in our observed facts.

Personal observation and the testimony of others provide in this way the subject-matter of empirical generalization, and the next step is that of forming a provisional explanation or plausible hypothesis to account for the problem to which they have given rise. Again, there are no rules for this; brilliant conjectures that are not merely spectacular, but along profitable lines are little likely to occur to a mind that is not already well informed of the detailed character of the data. Every explanatory generalization was once an hypothesis; in fact, it is substantially one and the same proposition that now is empirically confirmed as once was hypothetically entertained. It is precisely one of the privileges the sciences enjoy that their hypotheses are amenable to confirmation or rejection by appeal to observable fact, and this doubtless accounts in a measure for the astonishing rate of their development. The empirical scientist refuses seriously to entertain an hypothesis unless it is of such a character to permit of it being tested directly or indirectly by observation. (But when the problem to be explained is about supposed facts whose existence cannot by the nature of the case be established inductively nor be revealed to sense-perception, then it is obviously absurd to insist on empirical verification of hypotheses. This is largely the case, for instance, with the major problems of philosophy, and therefore we shall not be surprised to find progress towards their solution is slower in consequence.)

Scientific hypotheses that have been empirically confirmed not only explain the problem for which they were invoked, but also, as a rule, allow us to account for other facts with which we may have previously been acquainted but which have not so far been explained, or to discover additional facts that have not so far been observed. In this way, by means of one and the same attested generalization we are enabled both to explain and to predict. Simple enumeration of observed resemblances is sufficient to occasion the formulation of an hypothesis, but never, of course, to verify it. Argument by analogy may slightly increase the low measure of its initial probability, but the most difficult part of the explanatory task remains to be performed—that of adducing empirical evidence in a form suitable to demonstrate the truth of the hypothesis with an appreciably high probability. A genuine causal law, or assigned set of determining conditions for the occurrence of observable states or events will, if both "cause" and "effect" have been defined with sufficient determinateness, be completely reciprocal. In such cases we shall see, not only that the determinate cause gives rise to a particular effect, but also that that determinate effect follows from its determinate cause and no other. Such factual evidence adduced in support of an hypothesis does not turn up in the course of nature in its essential "purity," so to say, but is found intimately associated with other conditions which are irrelevant to the solution of the problem in hand. Hence, these irrelevant associated conditions must be eliminated to secure an isolation of the determining conditions that are relevant. All attempts to effect such an elimination are directed in accordance with two principles—firstly, whatever may be eliminated from a supposed set of determining conditions, without eliminating the facts to be explained, is not one of the determining conditions of those facts; and, secondly, whatever condition can be present among what are supposed as being determining conditions while the facts to be explained are themselves absent from the phenomena, is not one of the determining set. Both these principles presuppose that there is a cause for every event, and that the same cause or set of determining conditions always "produces" the same effect. So the general character of inductive investigation is to ascertain what are the determining conditions indispensable to account for the character of the phenomena to be explained. And it may assume two forms: (*a*) that of showing directly, by observation or experiment, that the elimination or quantitative variation of a certain antecedent is followed by the elimination or quantitative variation in the characteristic of the phenomenon to be explained; and (*b*) that of showing indirectly that, provided a certain condition remains constant, alteration in other conditions associated with it does not entail any alteration in the phenomenon to be explained. Thus a dependence of the phenomenon upon that invariant is established. Specific formulations of these two fundamental principles to meet the special exigencies of various subject-matters are referred to as the methods of "difference," "concomitant variations," and of "agreement." The two former are by far the more cogent. In some cases the researcher, through insufficient knowledge, or through the peculiar character of his data, may not have sufficient "control" over the phenomena to be explained, so that the empirical data cannot then be analyzed with enough thoroughness to exhibit them as precise instances of the character presupposed by the method. And it is exceedingly seldom that the evidence adduced in a piece of research exemplifies only one of these methods. One method is usually found to supplement another at different junctures in the evidential argument, and thus a higher probability is secured for the truth of the hypothesis.

Now, if the inductive methods are interpreted as being primarily means of *discovery,* then their worth will be severely limited for the reason that Keynes points out—namely, that "they assume experience can shape and analyze evidence in a manner and to an extent which is not in fact possible." But we shall not go to the same extremes as Campbell does in *What is Science?* (p. 76), where he maintains that these methods "are futile, because the problem they profess to solve is not one which has ever troubled any intelligent person. They tell us how, when we know that an event is the cause or effect of another, we may discover of which other event it is the cause or the effect; as a matter of fact, the crudest common sense applied in everyday life serves for the purpose." This condemnation arises from a misunderstanding of the character of these "methods"—the misunderstanding which is evident in the words "they tell us how . . . we may *discover* . . ." The inductive methods, far from being practical rules for *discovery*, are rather rules for *proof*, or statements of the general types of proof into which empirical evidence falls. Their function is not so much to discover evidence for us, nor simply to suggest the general character of the evidence requisite for proving an hypothesis. It is to demonstrate that certain evidence, *already acquired* by the researcher, being of such and such a character, *does* confirm his explanatory hypothesis. Differences of the inductive methods correspond to and are a consequence of differences in the character of empirical objects themselves. Factual evidence of a sort suitable to prove an hypothesis of co-variation among states of a certain class of objects will clearly have to be different from evidence which is sufficient to prove mere causal dependence between characteristics that are invariable and either wholly present in, or wholly absent from, their objects.

The one method that is implicit in these inductive principles is that of varying only one determining condition at a time. And these so-called methods are more usefully conceived as principles of demonstration or proof, than as instructions on how to make discoveries. They seem to be formal principles of proof applicable to different types of empirical subject-matter, and reached by a comparative examination of a number of accepted scientific proofs dealing with data widely diversified in character—in much the way I suggested in Chapter IV. that the conditions of valid deductive inference might be discovered for ourselves. So conceived, the strict demonstrative cogency of these principles is hardly a defect, but a merit, since it operates "normatively," as a standard that compels us to recognize where our empirical data falls short of, as well as where it approximates to, satisfying those conditions of proof.

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CHAPTER VII

THE BASES OF GENERALIZATION

Inductive investigation reaches explanatory generalizations from observed particulars. Typically, from an empirical generalization of the form, "All observed S's (or a selection of observed S's) have the character P," we pass to an assertion, with high or low probability, that "All S's whatever (irrespective of their 'place' or date, past, present, or future) will have the character P." The following will serve as a relatively simple example. We have observed that this, that, and the other sample of methylated spirit was inflammable, and we believe that all samples of methylated spirit, no matter when and where they are met with, will likewise be found inflammable. If we study chemistry we shall find that all samples will have this character because of certain constant properties which constitute, in part, the determining conditions. But in all such cases our direct evidence is restricted to a few instances with which we were personally acquainted, eked out, perhaps, with the testimony of others. But our generalization applies indifferently to all specimens of the fluid wherever and whenever encountered. Strictly, two assumptions have been made here (assuming we have not chemically analyzed the observed samples and satisfied ourselves that they are not really samples of other inflammable fluids)—namely, we assumed that the samples observed *do* possess the defining properties of methylated spirit, when we have only "authority" and not perceptual evidence of this, since we only have perceptual evidence of the inflammable character of our samples (and even this is reached inferentially). We *believe,* then, that they are samples of something having the defining characteristics of methylated spirit, and *observe* that they each possess another characteristic besides these. From this we pass to a further *assumption* that anything else possessing those same defining properties would possess this further property also, and we should affirm this belief justified on the ground of an analysis of, it may be, only one sample, where our findings are related to other relevant chemical knowledge already attained. Our principal underlying supposition, then, is this: admitting that *all* specimens have not been and cannot be directly investigated by the nature of the case, this limitation, nevertheless, should not prevent our accepting as true a generalization about the innumerable *un*examined samples, provided we can know fully enough the conditions which precisely determine the inflammable character of any one sample of the fluid.

And a moment's reflection will show us that the great mass of general beliefs we hold, often unconsciously, in daily life are generalizations which make these same assumptions. So, evidently, the probability of their truth will at least in part depend on the truth or falsity of this implicit assumption. Inquiry into what grounds, if any, can be found for believing this assumption raises philosophical issues which cannot be briefly discussed with advantage. The assumption which is, as above stated, a negative one, is usually formulated in positive terms and referred to as the principle of the Uniformity of Nature. But this title really applies not, as might be supposed from its affirmative appearance, to a very fundamental piece of *knowledge* we possess about the natural world, but really to a *problem,* which so far has not been solved nor even satisfactorily formulated. One interpretation of the principle or, more strictly, *postulate,* is that the natural world forms a system which, as a whole, remains identical and unchanging, changes occurring only within parts of it, and these changes being determined by its nature as a whole. Were the whole natural system fundamentally different in nature from what it is, the laws of the changes of its parts would be, in the aggregate, different in character from what they are now discovered to be. Such a postulate, both for its proper formulation and for its critical examination, would obviously involve an excursion into philosophy, for the issue partly turns on a discussion of contingency and necessity, and calls for a fuller analysis of the notion of "the natural world" than has yet been attained. The interpretation is useful, however, in emphasizing that there are really two important and distinct assumptions made by the principle of uniformity, and not only one. It is assumed both that the ground of natural changes is a system of some sort that persists unchanged through time, and that any change in any one "part" of the system is determinately connected with some particular sort of change in another "part." (This latter assumption we noticed briefly on p. 69.) The Uniformity postulate meanwhile remains a postulate—it is not self-evidently true or axiomatic like the laws of thought (for it can be denied without contradiction), nor can it be established as probably true by induction itself (for this would be circular), nor is it a provisional assumption, held only until it can be supplanted by a proved principle and then "scrapped." It is a proposition whose truth must be granted or allowed us if ever we are to reach explanations at all, since it lies at the basis of generalizations of every sort about existent objects and events, both the generalizations constantly made in daily life as well as those made by every empirical science.

The principle of Natural Uniformity, then, contains the assertion that changes are determinately connected, and this idea is given complete expression, according to some logicians, in the notion of Causation. But it is doubtful if this clarifies the issue, since "causation" itself is a conception the explanation of which is beset with difficulties. We can easily enough recognize *instances* of causation, occurrences that are called "causes" and "effects" in ordinary experience, but the term is so loosely used that we cannot be clear how it should be defined. We speak, for instance, of influenza causing absence from work and of causing death, of a war being an effect of avarice or ignorance, or of the ringing of a bell being an effect of pressing a button. For practical purposes all such descriptions are sufficiently clear. We can "know" a cause when we "see" one, but we do not know precisely how to define causation in such terms that we can be sure that, in this sense, there *are* causes in the world, nor that our definition exhausts all that is really "causal" in those situations. But if the notion of causation is introduced to supplement the Uniformity principle, it could do so only if we have been able to reach a really adequate definition, in order to find out whether it really *does* supplement it and is not merely mistakenly assumed by us to do so on account of its prevailing vagueness. Or it might be objected that it *could* not supplement the principle since it constitutes part of the principle itself. The truth seems to be that we cannot tell what "supplements" what until we have clear and adequate definitions in mind.

It may further be argued that the interpretation of the Uniformity principle already given (p. 74) is too extreme a statement of the character of our assumption. For, it will be said, firstly, that we may expect or hope to find "order" in nature without thereby having necessarily *assumed* it; or, secondly, that if we do feel it necessary to assert definitely a belief in natural orderliness, we may nevertheless *not* be prepared to say that nature is orderly "through and through." We could not perhaps prove the impossibility or the improbability that there exist "regions" that cannot be explained by any means whatever. According as we incline to either of these views, we shall want to substitute a more restricted interpretation of the Uniformity principle than that given on p. 74.

But we may say as regards the first point, even though we may quite well entertain in mind the hypothesis of natural uniformity, without necessarily having to *affirm* its truth (especially if, with Mach, we regard scientific generalizations as being merely "descriptive"), the fact remains that our generalizations in daily life are often also expressions of rational *expectation*, and that scientific explanations can also be grounds for deducing further rational expectations *only if we admit*, not merely to hypothetically entertaining "natural uniformity," but *that we are definitely assuming it* every day of our lives, whether we are aware of doing so or not. As to the second point, it is, of course, also possible that science, being always selective, only selects for investigation classes of facts which, *prima facie*, or in the light of knowledge attained, appear *likely* to exhibit order of some sort. Or, with M. Emile Meyerson, we may think that science is constantly revealing the existence of "irrationals" in reality, factors that resist all attempt at explanation, or which can only be "explained" at the cost of bringing to light still further previously unexpected "irrationals," and that the explanatory process generally really depends upon the existence of such inexplicable factors. Clearly, if this be true, again, we should only be prepared to accept a more restricted statement of the uniformity principle than our previous one. But natural uniformity to *some* extent, and in *some* sense, would still be assumed by us in scientific and practical affairs. So, to sum up, it seems much more probable that we do, and must, assume *some sort* of uniformity principle. But it is by no means clear exactly how we should state what we are assuming in all these cases. Here, too, then, the formulation of the problem is evidently halfway towards its solution; but general agreement upon its precise formulation has not yet been reached.

One important contribution in this direction, made in recent years by Dr. C. D. Broad and Mr. J. M. Keynes, is that called the principle of "Limited Variety." It amounts to the assumption that no natural object about which we generalize is so complex that its properties fall into an infinite number of independent groups, or that if there are any objects so complex as this, at least we have on some occasions a finite probability that the object we want to generalize about is not infinitely complex. Thus, with regard to some, or all, natural objects, it is maintained that there are groups of properties finite in number which are such that the possession of one property belonging to such a group entails the possession of some other in that same group. Here we evidently have a much less vague statement to consider than we have in the traditional interpretation of the Uniformity principle, though it is no less "general" or "wide" in its reference. "*Limitation*" of natural variety is the limitation of any one natural property to connection with other properties *belonging to its group*, and the denial that it may be connected equally with natural properties belonging to other groups. "*Variety*" refers to the assumption that natural properties "hang together" in groups, the possession of any one property determining the possession of some other property within its group.

A superficially similar doctrine is the traditional one of "natural kinds," or "species," but research in evolution has shown there was no reason to believe in the existence of fixed, unmodifiable species. There does, however, seem good reason to believe that there are natural properties ordered into groups in the way suggested by the Limited Variety principle. A natural law on such an hypothesis will be the fact that the possession by a natural object of one property of a given group entails the possession by it of a certain further property belonging to the same group. Explanatory generalizations will then simply be propositions about such natural laws or facts of entailment.

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After this survey the reader will be in a position either to follow up "separate" topics of logic arising out of a fuller study of points raised in the chapters of this booklet (and the "References" appended to each chapter will guide him to suitable reading), or he will probably be better advised to study some more advanced treatises in their entirety. An excellent course of logic would consist in a careful study of the classic treatise on the subject in our day, W. E. Johnson's *Logic* in three volumes, along with J. N. Keynes' *Formal Logic,* J. A. Venn's *Empirical Logic,* A. D. Ritchie's *Scientific Method,* and J. M. Keynes' *Treatise on Probability.* When this solid programme of reading has been accomplished the reader's own inclinations will determine the direction of his further study. I set out a further list of books on logic written from different "approaches":

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